

**N. H. Azrin (1953–1955)****PIGEON LAB NOTABLE EXPERIENCE**

B. F. Skinner's published views on punishment were well known at the time of my arrival in September 1953 at Harvard University, which I attended for the sole purpose of studying under Skinner. He had coauthored some studies of punishment earlier with W. K. Estes, but had devoted virtually all of his other animal research to the study of positive reinforcement. He was opposed to the use of punishment to influence human behavior, a view strongly expressed in his books *Science and Human Behavior* (1953) and *Walden Two* (1948), and indeed shared generally by psychology at large.

My own view at the time was that the strong opinions and ethical views regarding punishment had prevented the serious study of that process to the same extent that was true of positive reinforcement. I believed that punishment deserved more study; more specifically I believed that such study should address some of the same factors, such as the schedule of presentations, as had been found by Skinner to be so important with positive reinforcement.

In the unusual tradition of the Pigeon Lab, no prior approval was needed by graduate students, such as I was, for the specific subject matter of the studies I would be conducting. This in spite of the reality that we students were using Skinner's facility supported by external funds explicitly awarded to him. Looking back at this arrangement, I am even more impressed with this freedom to select and design one's own study because I intended to

use the studies as my PhD dissertation. In the previous and subsequent universities with which I have been affiliated, the doctoral student is required to submit a formal, lengthy, and scholarly proposal prior to collecting any data.

The crucial moment for me was a year later, when I arranged a meeting with Skinner to present my data in the form of cumulative records to show what I had found regarding fixed and variable schedules of punishment. I expected him to trivialize if not strongly disapprove of these punishment studies, and I fully expected him to qualify his acceptance of the value of the findings to be sufficient for a doctoral dissertation.

To my surprise and delight, his reaction was one of unqualified delight and excitement and encouragement for me to continue these studies and use the findings for my dissertation. I felt at that moment that my reasons for coming to Harvard to study with him were well founded. He represented the ideal of a behavioral scientist, excited about the discovery of functional relations of behavior and uninfluenced by speculative and unfounded theoretical expectations.

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**Terry W. Belke (1988–1993)****CONTEXT MATTERS: MY EDUCATION AT THE HARVARD PIGEON LAB**

I regard my years of graduate training in the Harvard Pigeon Lab under the supervision of Richard Herrnstein, Gene Heyman, and Will Vaughan as the period of greatest intellectual development in my academic career. The lab fostered intellectual inquiry by combining highly motivated graduate stu-

dents with everything needed to investigate any question of interest to the student. Two lofts of pigeons, a colony room of rats, operant chambers with two, three, and even five keys located in several rooms, Digital PDP-8a® computers, a room filled with racks that interfaced the computers to the chambers, and

a workshop were available for all to use. Students were introduced to the lab, the PDP-8a, and SKED® by Vaughan and then were set loose to do as much or as little as they wanted. This was the context within which my avid interest in the study of choice grew. The intellectual expertise provided by Herrnstein, Heyman, and Vaughan guided and challenged my understanding of matching, melioration, and variables that influence choice.

Interactions among the graduate students provided another source of intellectual development. During my time in the lab, the finer points of theories such as melioration were vigorously debated by the students into the wee hours of the night with the vigor supported by a postmidnight pizza hastily ordered during a break in the debate. Presentations of research and discussions of articles occurred on a weekly basis in the Behavioral & Decision Analysis Research Seminar, otherwise known by the participants as the pigeon staff meetings. These meetings provided a forum for graduate students to present their research and obtain a critique of their work. In addition, guest speakers such as Irene Pepperberg and Herman Samson presented their recent research to the group. Visiting scholars Stuart Vyse and Ben Williams worked alongside the graduate students and interacted with them. John Cerella was actively investigating the features that pigeons used to categorize objects in the lab.

The Pigeon Lab was where I conducted the research that initiated the line of intellectual



Across the Charles River with the Houses of Harvard in the background.

inquiry that I pursue to this day. The lab provided a context for the study of choice. I had previously been mentored by W. David Pierce, who had studied activity anorexia in terms of the effect of food intake on the reinforcing value of wheel running. The marriage of context and background yielded an extension of the matching law to wheel-running reinforcement. I suspect that this is but one of many lines of intellectual inquiry in our field that can be traced to this lab.

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### Robert A. Boakes (1963–1966)

#### FROM PROGRAMMED INSTRUCTION TO PIGEONS

In September 1963, I arrived as a graduate student at Harvard. Three years later I returned to England. This period was one of exciting change in the local academic and political scene. Harvard students could take courses at MIT, so the rich intellectual climate included seminars with Noam Chomsky and the budding philosophers and psycholinguists who had gathered around him. In the streets one might see Joan Baez crossing Harvard Square or an early antiwar rally in Boston addressed by Chomsky.

I came to Harvard because of a meeting with Fred Skinner in Cambridge, England, when I was an undergraduate. I talked to him about my interest in teaching machines and he told me about the Center for Programmed Instruction (COPI), encouraging me to apply to Harvard. At Cambridge the influence of cognitive psychology was already strong, mainly because of the vigor of the Applied Psychology Unit under the leadership of Donald Broadbent. But, unlike in the U.S., cognitive psychology in the U.K. was combined

with an interest in learning. Broadbent's relatively unknown book, *Behavior* (1961), influenced me as an undergraduate as much as any book by Skinner. Whereas I came to Harvard with an interest in applications of operant conditioning but skepticism about Skinner's theoretical ideas, many fellow graduate students arrived with an already strong commitment to radical behaviorism.

In 1963 the Psychology Department was still housed in the basement of Memorial Hall. I remember it as a place dominated by the long hours of study needed for the proseminar and preliminary examinations. S. S. Stevens exerted a major influence. He continued traditions established by Edwin Boring, including the importance of history, of psychophysics, and of working at least 70 hr a week. More generally, 1st year students were taught respect for clean, honest data and suspicion towards statistics. Only Jerome Bruner emphasized theory.

During the 1st year, my only research work was at COPI, under the supportive supervision of Jim Holland. This work brought me into contact with psychologists in the Boston area who were developing applications of operant conditioning. Many of these—Murray Sidman and Nathan Azrin, for example—attended the weekly pigeon staff meetings held in the department. Even to a newcomer like myself, however, there appeared to be a widening gap between the interests of these "outsiders" and those of researchers within the department. By the end of 1964 it became rare for any outsider to attend the meetings. I was aware that this represented a recent shift from the time when there had been more commonality of interest between graduate students within the pigeon lab, such as Catania, Reynolds, and Terrace, and researchers in nearby labs.

The experiments I was conducting at COPI became unsatisfying. John Staddon suggested that, if I were interested in understanding fundamentals of learning, it would be more productive to run experiments with pigeons than with Harvard undergraduates. A little later I joined the Pigeon Lab. When the department moved to the new William James Hall, my relay rack joined the others that filed across Kirkland Avenue and ascended to the new Pigeon Lab on the seventh floor. The change from a horizontal basement warren to

a vertically layered department reduced the interaction between the Pigeon Lab and other labs. There were fewer discussions between Pigeon Lab students and the other large and assertively articulate group of graduate students, those working with George Miller and other members of the new Center for Cognitive Psychology.

In hindsight, the experience of my generation of students in the Pigeon Lab was highly unusual. Notably, there were so many of us—at least a dozen in a given year—all with a single adviser, Dick Herrnstein. We were given a great deal of freedom to get on with whatever experiment we thought worthwhile. For many of us, contact with Herrnstein was irregular. The unspoken attitude seemed to be that, if we were good enough to get into Harvard and to complete the prelims, we were good enough to choose our own topic and pursue it sensibly. When I sought his advice, Herrnstein would give good value. However, the discussions I remember best were about more general issues. These were always challenging, enlightening, and good humored, even if we rarely agreed. Most of the practicalities of experimenting I learned from fellow students during the long days in the lab. The more senior students had already completed their experiments but, nevertheless, were very willing to spend time helping novices. On a day-to-day basis the students just a year ahead—Bill Baum, Phil Hineline, Al Neuringer, Howie Rachlin and Richard Schuster, for example—were always there to help or get involved in some new discussion.

Again with hindsight, this was an especially productive period for Herrnstein. He was involved in research that led to the matching law (e.g., Chung & Herrnstein, 1967), mounting his challenge to the two-factor theory of avoidance (Herrnstein & Hineline, 1966), developing his ideas on superstitious behavior (Herrnstein, 1966), and publishing his first paper on perceptual categorization in the pigeon (Herrnstein & Loveland, 1964). Unsurprisingly, students involved in these projects saw more of him than those like me who were working on different topics. Choice behavior seemed to be his dominant interest. Opinion in the lab was divided as to whether this was or was not the most important problem in psychology. Personally I never really appreci-

ated the interest in quantitative description as an end in itself. Herrnstein seemed ambitious for his matching law to emulate Stevens' power law; despite compulsory 1st-year immersion in the latter, I had never believed that this principle was particularly helpful for understanding perception.

I do not remember ever seeing Skinner in the Pigeon Lab. He was not required to give any undergraduate courses, but did offer a graduate seminar some years. The only one I was able to take was disappointing in that it covered very familiar ground. The highlights were challenges by senior students. Rachlin pointed out experiments on punishment that contradicted Skinner's long-held views on the matter. Staddon argued the merits of control theory as an approach to certain problems in behavior. Both were rejected in almost automatic fashion. Unlike Herrnstein, Skinner showed limited interest in engaging intellectually with graduate students. On the other hand, alone among the faculty, he was hospitable, on several occasions inviting students from the Pigeon Lab to his home. He seemed

more open to discussion at these events than in his seminar. I never regretted that he had persuaded me to apply to Harvard, even though I had little contact with him once I was there.

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## Peter B. Dews (1953-1956)

### A VIEW FROM AN OUTSIDER

I joined the Department of Pharmacology at the Harvard Medical School in January 1953. Within a short time of arriving, Otto Kraye, head of the department, said that he had received a letter from one B. F. Skinner over the river saying that he had methods that he thought may be of interest to pharmacologists. He also sent some pigeon grain! I never saw the letter, but it may still be in the archives. Neither Kraye nor I had ever heard of Skinner, but I made an appointment to visit him and went over to Cambridge with Peter Witt from Switzerland, later known for his work on the effects of drugs on spiders' web making, who was spending a year in the department. I have described elsewhere how we chatted and Skinner turned us over to Charlie Ferster for him to show us around the lab, and my immediate fascination. Witt was less impressed and said he (Skinner) talks like J. B. Watson.

Before January was out, I was a regular at-

tendee at the weekly pigeon staff meetings and had become acquainted with William Morse, Richard Herrnstein, Douglas Anger, Donald Blough, Ralph Gerbrands, Rufus Grason, S. S. Stevens, E. G. Boring, E. B. Newman, George Békésy, and everybody else in that part of the basement of Memorial Hall. I was welcomed into the communities. I do not remember being actually invited to attend the pigeon staff meetings, although Ferster probably said we have Friday afternoon meetings you might find interesting. I do not find the welcome surprising. I have worked in half a dozen labs and have felt welcome in them all. It is one of the rewards of a life in research that if you go into a lab and show a respectful interest in the work in progress you will be welcome.

By 1953 the Pigeon Lab was a mature lab, with funding from the Office of Naval Research, which in those days was funding research that would be later taken over by NIH

as the budget of NIH grew. Also, by the end of January, drug experiments had started in the Pigeon Lab, made possible by Ferster, and before long I was making drug solutions in the medical school for other people in the Pigeon Lab, notably Morse and Herrnstein. Naturally, I wanted to do experiments in my own lab at the medical school, but not, of course, on pigeons. But Ferster offered me a complete setup for pigeons to take over. The temptation was too great, so pigeons arrived in the medical school, to nobody's subsequent regret.

The people in the Pigeon Lab were hard working and enthusiastic and knew they were in an excellent lab. But I do not think they fully grasped what epoch-making discoveries they were making; perhaps not even Skinner

realized, as judged by his writings during the Pigeon Lab era compared to his pre-1950 writings. It is well known that *Schedules of Reinforcement* (1957) met with a cool reception. Skinner had spent little time cultivating the favor of his psychological bretheren; indeed, he was openly contemptuous of much in contemporary psychology. But, then, it also took time for Gregor Mendel to be appreciated.

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#### Edmund Fantino (1961–1964)

##### THE NURTURING OF A BEHAVIOR ANALYST

The Pigeon Lab is best understood in terms of the intellectual context in which it was embedded. The Department of Psychology at Harvard, at least in the early 1960s, was partitioned into three enterprises: the newly minted Cognitive Center, spearheaded by George Miller, a founder of the "information-processing revolution"; the Psychophysics Laboratory, directed by S. S. Stevens, developer of the psychophysical law; and the Pigeon Lab. When I arrived, fresh from a BA in mathematics, I was deciding between psychophysics and cognition. I had little interest in behavior analysis, regarding it as too restrictive and narrow to deal with the rich tapestry of human behavior. Two and a half years later I turned in a doctoral dissertation on risky choice in the pigeon and was a committed behaviorist. I believe my transformation was a function of the structure of the department's graduate program and the nature of the scientists working in the Pigeon Lab at the time. These influences, the basis of my commentary, undoubtedly affected others as well. For throughout the 1960s, a large proportion of the bright, undecided, incoming students gravitated towards the Pigeon Lab.

A critical aspect of the graduate program was its emphasis on breadth. All 1st-year students were required to conduct a research project in each of the three areas. We did this in groups of three so that three reasonably substantial projects could be completed in one academic year. For the pigeon project, Bill Krossner, Joyce Shaw, and I (directed by George Reynolds) studied sound localization. This project involved a greater degree of methodological complexity than those we did in the other laboratories, and the individual data seemed more meaningful than those from the other projects. Suddenly I grasped the possibility that behavioral techniques offered solutions for problems of broad general interest. My career choice had been complicated. Breadth was also insured by the department's preliminary exams. This was a set of four 3-hr exams, offered on consecutive days, before the start of classes in the fall semester. Typically one had to grapple with sensation and perception; learning and motivation; thought and cognition; and physiological psychology. Preparing for these four exams was arduous, and in all cases involved an appreciation of the field's historical un-



derpinnings as well as current developments. The learning and motivation exam was not simply a review of operant conditioning. Traditional learning theories were emphasized as well as developments in, say, mathematical learning theory. In 1963 I don't recall a single question on behavior analysis, an indication that even the Pigeon Lab academics were emphasizing breadth, assuming that we would pick up knowledge of conditioning in seminars and in research. This breadth put us in good shape for subsequent teaching positions. (In my 1st year at Yale, I was required to teach sensory processes, perception, motivation and statistics; thanks to the prelims and to an exam requirement in probability and measurement, I was able to do so without embarrassment. In fact, when I asked to teach a graduate seminar in learning the next year I was turned down and was offered a graduate seminar in perception instead—one of my fondest teaching experiences.)

The breadth of the program in experimental psychology not only permitted students to gain a strong background in the discipline but also forced them to sample the strengths and weaknesses of the approaches offered. It seems that in those days the Pigeon Lab came out best. One reason was the presence of dedicated young researchers who were around at all hours creating an intellectually exciting atmosphere. Although we all profited from interactions with Skinner and Richard Herrnstein, in 1961–1962 the constant presence of recent Pigeon Lab PhDs George Reynolds and Charlie Catania helped promote interest in the operant approach. Of the seven entering students in the fall of 1961, four of the six who continued chose to emphasize research in the Pigeon Lab (Lois Hammer, Bill Krossner, John Staddon, and me). Although Catania and Reynolds left in the summer of 1962, the next batch of incoming students had this committed group of 2nd-year students to provide enthusiasm for the Pigeon Lab. One of those incoming students (Howie Rachlin) told me that was a major reason why he chose the Pigeon Lab. Interactions among students were fostered by the “living arrangement” of six students sharing an office.

The department's seminars were scheduled in the evenings, and the arguments and discussions that occurred there often continued at the watering holes of Cambridge until clos-

ing. What wasn't available at Harvard often was at MIT. Thus, several of us took brain and behavior seminars with Hans Lucas-Teuber there. In addition, Allen Neuringer, Richard Schuster, and I took a physiological psychology laboratory with Steve Chorover at MIT that occupied our Thursday afternoons and part of our evenings (after which Schuster returned to his family and Neuringer and I repaired to Jenny's in the North End where we would review the day's activities over pasta and an authoritative house red).

But the seminar that most influenced me came in my 2nd year. One of the articles in this learning seminar, taught by Jim Holland and Brendan Maher, was Chomsky's (1959) infamous review of Skinner's *Verbal Behavior* (1957). Skinner had assigned much of *Verbal Behavior* in a seminar the prior year. Although I found the book interesting, I felt that it fell short of an adequate explanation. My reservations about the potential applicability of behavior-analytic principles to human behavior had not been assuaged by my reading of the book. Reading Chomsky's review changed everything. After getting past his inappropriate diatribes against drive-reduction notions, I began examining Chomsky's claims regarding the inadequacy of Skinner's account. I found myself defending the approach, often after going back to the text and reviewing a relevant portion. By the time I was through I had come to the conclusion that *Verbal Behavior* was a grand achievement and that behavior analysis had the potential for answering the largest questions about human behavior, a position I hold to this day (e.g., Fantino, 1998a, 1998b; Stolarz-Fantino & Fantino, 1990, 1995). I have never looked back.

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## Edward J. Green (1949–1953)

### REMINISCENCES OF A REFORMED PIGEON PUSHER

It is impossible for me to separate my work as a research assistant in the Harvard Pigeon Lab from my experiences with Fred Skinner. Before I joined the lab, I was told by one of his former assistants at Indiana University what I should expect. He warned me that I would be given responsibility to find ways to achieve a particular behavioral objective by whatever mechanical, electric, or other means I could find, and that it would be up to me to find ways to succeed at it. One had to grasp whatever Fred had in mind by way of a research objective and then find a way to achieve it. The emphasis was on initiative and originality. No one could complain that his assistants worked within a straitjacket. The atmosphere of freedom of inquiry in which we all worked in that environment was the salient feature of those years for me. As a 2nd-year graduate student, I completed two studies using rats and pigeons before settling on my thesis problem. Both of these were subsequently published, and when I apologized that my acknowledgment for support from the lab had been lost somewhere in the shuffle, Fred replied reassuringly, "We don't exact tribute here." Independence of effort was not only encouraged; it was expected.

The program of the Pigeon Lab gave experimental psychology its flagship research in the field of learning. Not apparent at the time to those of us preoccupied with the effects of schedules of reinforcement, species-specific behavior, differential reinforcement of low and high rates, and rigging ping-pong demonstrations was the subtle influence of Skinner's concept of the operant, which implicitly defined what a true science of human behavior must eventually become. Although the research program shifted focus several times from studies of the effects of schedules of reinforcement to implications of aversive control, the fine structure of visual discrimination, drug effects, and the like, the pre-

vailing theme was that of inductive pragmatism. Regardless of the occasionally impressive swirls of theoretical obfuscation that typified those times, we all knew that what we were doing "worked." That, plus Skinner's oft-repeated observation, "The subject is always right," kept us close to the language of the data. Fred Skinner was impatient with abstract philosophical arguments mainly because he understood their implications so clearly. I recall how the objection by some of our colleagues that Gödel's proof challenged the validity of empirical research was dismissed with a snort to the effect that, like it or not, the experimental approach worked, and so it did. On another occasion, Skinner expressed irritation that another colleague had once made the point that despite his protestations, he did indeed have a theory. His reply was that if thinking the sun is going to rise tomorrow because it always has is a theory, then he guessed he did have one. In any case, he was never opposed to theory as such, only to bad theories.

In the years following, I have often found myself describing the work of the lab to my own students as a place where anything could find its place into the body of science, no matter how unexpected. There was no overriding preconception that ruled where research should or should not go. All that new facts needed for admission to scientific respectability was that they meet minimal operational requirements. New concepts had to be publicly replicable to be verified and accepted.

Programmatic research of the kind pursued in the Pigeon Lab is now rare or impossible for many reasons. One is the "flight from the lab" that Skinner himself decried. Psychology is a field entranced with pop culture and quick fixes. Cognitive science, when it is not resurrected structuralism or committed to proving that computers think just like

we do, is what Skinner once called methodological behaviorism. Its practitioners still are in the business of inventing intervening variables: Only the names of the intervening variables have changed. "Theories" that sprout like mushrooms, rather than basic research, are currently the popular roads to fame and tenure. The operational details of research on the behavior of nonhuman organisms offer little to practitioners who are practically concerned with knowing which of their customers is most likely to jump off a bridge this week, and in the present climate of "relevance," funds to pursue basic lab research are hard to come by. The Pigeon Lab was initially supported by money given for answers to the question of how most effectively to steer a bomb or nuclear missile to a remote target. Those who funded the Pigeon Lab were almost certainly innocent of the very real advances in the study of behavior that their grants subsidized.

Of course all of this activity went forward with our implicit assumption that something like Newtonian determinism was the appropriate paradigm for all scientific inquiry. Nonetheless, day-to-day observations contin-

ually revealed that behavior, whether that of rats, pigeons, or humans, violated one of the prime implications of ontological determinism; namely, that the behavior of an organism reverts to the steady state that prevailed before an intervention. If there is a fundamental truth about operant behavior, it is that it is a constantly evolving process characterized by an unending series of divergences. No organism can ever be the same as it was before its behavior was selectively reinforced. Skinner certainly did not propose the concept of a class of "emitted" behavior to anticipate quantum mechanics. The concept was simply the honest concession of the fact that we do not know, nor *can* we know, the specific eliciting stimulus that is responsible for the occasion of a particular operant response. The research conducted in the Pigeon Lab would be valid even if the new physical paradigm had been recognized and broadly accepted as the appropriate paradigm for behavioral research. In this respect, its work stands alone as a model for the new century and beyond.

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### Gene M. Heyman (1970–1976)

#### THE HARVARD PIGEON LAB, 1970–1998: GRADUATE STUDENTS AND MATCHING LAW RESEARCH

In 1970, the year I began graduate school, the Pigeon Lab occupied about half of the seventh floor of William James Hall. The animal colony took up the center room, and the shops and "running" rooms, filled with experimental chambers and relay racks, formed the periphery. The heart of the lab was the collection of relay racks with their electromechanical counters, steppers, clocks, and timers. Linked by relays and wires, these devices counted behavior and doled out rewards. It looked like science but also a little like a Rube Goldberg cartoon. In the spirit of the latter image was the laboratory legend that Skinner once tried to dampen the action on his feeders by coating them with Karo® syrup.

#### *Graduate Student Education and Interest in the Matching Law*

In the Pigeon Lab graduate students had free rein. We had easy access to equipment and animals and pursued our interests with little overt direction from the faculty. For a while the lab technicians, paid by Herrnstein's grants, even ran our experiments. We weren't apprentices but new researchers. Under this laissez faire educational system, research projects were varied and sometimes idiosyncratic. In the 1970s they included projects on autoshaping, taste aversion, delay of reward, concept formation, visual discrimination, and foraging. The matching law, however, in its encompassing single- and con-



current-schedule form, was new, and its generality and predictive powers attracted the interest of many of the students who showed up to study behavior in the 1970s.

Herrnstein's first general paper on the matching was published in 1970. I had seen the equations the year before in Dick's undergraduate behavior course (Motivation and Action). In the first lecture, Dick promised an analysis of behavior built entirely on observable regularities, "as if we were visitors from Mars and had no assumptions about the inner life of behavior." The course's guiding theme was that behavior was a function of its consequences. Near the end of the semester, we learned that the rule for how consequences governed behavior was the matching law.

For those of us working on matching, a division of labor naturally emerged. Peter de Villiers and Hal Miller extended the equations to new situations. Peter developed a matching law description of avoidance and punishment (e.g., de Villiers, 1974), and Hal demonstrated that Bill Baum's generalized matching law (1974) predicted preferences between novel combinations of qualitatively different reinforcers (Miller, 1976). Jim Mazur and Lexa Logue found links between the matching law and other choice theories. Jim rewrote Premack's theory of reward from the perspective of the matching law, substituting quantitative for qualitative predictions (Mazur, 1975). Lexa conducted novel studies based on the formal similarities between the matching law and signal-detection theory (e.g., Logue, 1983; and see Davison & Tustin, 1978). Arturo Bouzas and I found that the matching law described the frequency of polydipsic drinking (Heyman & Bouzas, 1980). Will Vaughan focused on the relation between matching and reward maximizing (e.g., Vaughan, 1981). And Drazen Prelec, who started working in the lab as an undergraduate, derived a reinforcement feedback function for multiple reinforcement sources that tested a basic principle of formal choice theory, the constant ratio rule (Prelec & Herrnstein, 1978).

In the 1980s, Drazen and Will collaborated with Herrnstein on the idea that matching was the result of a simple, myopic form of reward maximizing that they called *melioration* (Herrnstein & Prelec, 1991; Herrnstein & Vaughan, 1980). During this period most of

the lab research was on melioration and the relation between the matching law and microeconomic principles. Because of the overlap with economic theory, these studies often used human subjects (Herrnstein, 1991). The experiments applied methods of the earlier animal studies to economic questions. The results serve as one of the early chapters in behavioral economics, a burgeoning new field that applies experimental methods to economic questions (Herrnstein, Rachlin, & Laibson, 1997).

I do not recall that we ever discussed why we found the matching law a compelling topic. My reasons, which are likely similar to those of the other graduate students, were its generality, predictive power, and simplicity. We played with the equations, deriving new, more complex models, or substituting real numbers for parameters that predicted new results. The predictions led to new experiments, and often enough the pigeons and rats agreed with the math (see publications by Mazur, Prelec, Vaughan, and me). Very exciting stuff, especially when the domain is behavior, a subject matter that is not usually the focus of mathematical description.

#### *Enter Computers*

In 1970 most of the experiments were controlled by electromechanical relay racks. However, Bill Baum and an MIT undergraduate, Allen Razdow (who later helped develop mathematical software, e.g., MathSoft and MathCad), created a real-time software program for running operant experiments. The computer, a PDP 9®, resided on the 12th floor of William James Hall and was connected to the Pigeon Lab by cables that ran through five floors of offices and classrooms.

Baum and Razdow called their language OCSYS. It consisted of little more than a clock, timer, and the PDP assembly language commands, such as "move the accumulator left," "deposit in displaced memory location 8," "retrieve from direct memory location 1005," and so on. The program for my first experiment required more than 2,000 lines of instructions (Heyman, 1977<sup>1</sup>). A later version of the same experiment, written in MED

<sup>1</sup> Heyman, G. M. (1977, March). *Reinforcing deviations from matching*. Paper presented at the meetings of the Eastern Psychological Association, Boston.

PC®'s state language (Heyman & Tanz, 1995) required just a few hundred lines.

With OCSYS it was possible to program dynamic reinforcement contingencies that mimic processes such as tolerance and satiation. For example, for my 1st-year project, I arranged a contingency that differentially rewarded behavior as a function of measures that were recalculated with each new response. The logic of this contingency was beyond the capacities of electromechanical equipment, but was well within the scope of even a primitive computer. Now, dynamic real-time contingencies are familiar and have helped identify the relationship between matching and reward maximizing (Heyman, 1977<sup>1</sup>; Heyman & Tanz, 1995; Vaughan, 1981). As for OCSYS, it remained a local language and disappeared not long after simpler languages, such as SKED®, became available.

#### *The Pigeon Staff Meetings*

In weekly lab meetings we presented new results and new equations. It was an intellectual free-for-all, with no special deference afforded the faculty or personal feelings. However, Dick Herrnstein's imaginative and insightful responses to new data and new models often led the discussion. His comments came with humor and anecdotes, and he was as quick to see the positive features of a research project as well as what rested on untested assumption. These meetings often had visitors. In the early 1970s they included George Ainslie, David Premack, Jock Millenson, and Herb Terrace. Skinner never came to the meetings while I was at Harvard, which probably reflected the lab's shift to theoretical and quantitative accounts of schedule behavior. Conversely, most of the graduate students of my generation were not that interested in the philosophical and methodological issues that Skinner championed. This distinction persists. For instance, graduate students of the 1960s went on to publish widely on behaviorist views of psychological phenomena, whereas those of the 1970s have not.

#### *1989 to 1998*

I returned to Harvard and the Pigeon Lab in 1989 to take a position as assistant professor (completing a chain of junior faculty associated with the Pigeon Lab that included

Herrnstein, Rachlin, Baum, de Villiers, and Mazur). At about this time, Dick stopped running animal experiments. The pigeon staff meetings became less frequent, and with Herrnstein's death in 1994, they stopped. However, matching law studies continued along with research in psychopharmacology and drug self-administration. The graduate students included Terry Belke, Nancy Petry, Larry Tanz, Jamie Taylor, and, at times, Bill Reynolds and James Roach. In addition, there was a constant stream of undergraduates in the lab, working on senior honors theses or special research projects.

#### *The Contingencies of Graduate Training in the Pigeon Lab*

There is an irony at the core of this story. In the cauldron of reinforcement contingencies, the students could not have been freer from environmental constraints. Adequate resources were available for the asking, and they came with no strings attached. To be sure, many graduate students worked on faculty-related projects, but this was not built into the program, and those who pursued independent interests had equal access to the lab. Yet, year after year, students undertook ambitious behavioral studies, producing a steady stream of PhD theses and publications. Zuriff, who was a student in the lab in the late 1960s, suggests that we were reinforced by the orderly results and sense of discovery (personal communication, August 2001).

The Pigeon Lab came to a nominal end in June of 1998. Over the years it served as a congenial and supportive home for newly minted behavioral scientists. Lab graduates have continued to be productive, training new generations of behavioral scientists and publishing widely on behavioral phenomena. The dynamics driving this output are, I believe, the inherent orderliness of behavior, the inherent curiosity of the students, and an environment that supported the healthy mix of natural order and curiosity.

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## Philip N. Hineline (1962–1966)

### THE HARVARD PIGEON LAB IN CONTEXT

For me, to remember the Harvard Pigeon Lab is also to remember its context, the PhD program in psychology. When I settled into the basement of Memorial Hall in the fall of 1962 it soon became clear that a major priority would be mere survival. The results of the notorious annual preliminary exams had just been announced, leaving several students looking smug and comfortable, whereas the demeanor of some others suggested a shrinking presence. Survival was also going to require attending to some dimensions I'd never thought of. I quickly got into trouble by trading desk chairs with someone, only to be informed by Didi Stone, S. S. Stevens' secretary, that my chair had been purchased on a Stevens research grant and I was not at liberty to reallocate it. She did let me know, however, that I was welcome to continue using her grand piano in the nearby hallway (after hours, of course). That was the pattern: Relationships were universally cordial with music or skiing under discussion, but in academic matters we were acutely aware that there were two major fiefdoms in the basement of Memorial Hall, clearly demarcated by invis-

ible boundaries. S. S. Stevens, psychophysics, and the power law reigned at one end; Skinnerians with relay racks generating intricate patterns of behavior on reinforcement schedules were at the other end. Békésy's lab and the department office provided neutral zones on the back and front hallways, respectively.

Smitty Stevens (I found it easier to call Stevens "Smitty" than to call Skinner "Fred") became an even more imposing presence on the first meeting of the proseminar, when surveying the 13 newcomers arrayed around the massive table, he commented, "There are too many people in here." That first semester's heavy dose of sensory psychology was not what I had come to graduate school for, and it was several years before I recognized the value of what I had learned then. Billy Baum recognized the value more quickly, applying power functions to behavior in formulating the generalized matching law. In any case, our number had shrunk to 12 by the 3rd week, and one or two more disappeared by the end of the year. By the first midterm, it had become abundantly clear that I was in extremely formidable intellectual company.



Philip N. Hiney in the early 1970s, photographed by his first graduate student, F. G. David.

Although there was good-natured competition among us—as when Rachlin and Schuster fired obscure spot-questions at each other (Who *was* von Hornbostel, anyway?)—I never had the sense that one person's success implied another's failure. The standards confronting us may not have been entirely objective, but they seemed more absolute than relative. Scholarly cooperation was explicitly encouraged; for example, we were expected to duplicate and distribute our term papers to all participants, and these proved invaluable in studying for prelims. The department charged us a nickel per ditto stencil used for this purpose, though, which Rick Schuster protested by typing single-spaced without margins either at sides or at top and bottom. Such was student rebellion in those days.

The biggest challenge was to come up with a good experiment of one's own. A common sight during late afternoons was a queue of students outside Dick Herrnstein's office, each waiting a turn to discuss ideas or progress. My own "first acceptable idea" was traceable to a reading of Sidman's *Tactics of Scientific Research* (1960) as an undergraduate. Sidman had repeatedly used the "warm-up ef-

fect" in avoidance as an exemplary puzzle for discussing research strategies, but none of his proposed experiments struck me as really getting at the phenomenon. With Dick's approval of an initial experiment, I spent a whole summer trying to get animals to avoid—which gave me a new idea regarding what was going on during the warm-up and led to the experiments for my dissertation. Meanwhile, Doug Anger had published an interpretation of avoidance that placed heavy emphasis on evidence for temporal discriminations in the animals' performances. Because I already was the person studying avoidance, Herrnstein introduced me to a fancy five-interval resettable timer that quickly found its way into some exploratory experiments and prompted several weeks of puzzling over how to arrange or recognize randomness of events in real time. Eventually I stumbled into a way to enable a rat to postpone brief shocks without affecting their frequency of occurrence, and Herrnstein devised a way to achieve shock-frequency reduction in a procedure that lacked temporal regularities. Thus a collaboration evolved; the Herrnstein–Hiney relationship was always a very formal albeit cordial one, and Dick was very generous with his ideas. The "short-term versus long-term" issue emerged in other people's thinking as well, notably in the "self-control" conception introduced by Ainslie and Rachlin.

Once an experiment was under way, presentations of data at pigeon staff meetings were a major venue for discussing each others' work. I have the impression that they were a custom that had lapsed for a few years. Their initial resumption included colleagues from across the river—Murray Sidman, Bill Morse, Roger Kelleher, and Peter Dews—but they soon became mainly an activity of our laboratory group. Of course, discussions of experiments as well as conceptual issues were not confined to formal meetings. On one occasion, an argument over the algebraic definitions of positive and negative acceleration interrupted a basketball game for at least an hour. It didn't interfere with the beer-and-harpsichord session at Al Neuringer's place afterward, though.

Skinner attended pigeon staff meetings only occasionally. What impressed me most about his participation was his discerning how to clean up an experiment, making sug-



gestions that nearly always made the work more incisive. I regretted the fact that he spent most of his time away from the lab, writing about abstract issues rather than doing experiments. Still, despite his scarcity around the lab there were a few special opportunities to learn directly from the great man. Several of us were enlisted as exam graders for the final edition of his large undergraduate course, which entitled us to sit in on all the lectures. He also taught one graduate seminar in which a manuscript-in-progress was placed on the table—very exciting stuff, concerning issues like what is at issue when we speak of “seeing that we see.” He was the sole examiner for one of my preliminary examinations, on the psychology of music (how that came to be is a story in itself). But the best lesson in mentoring came on the day of

my final oral exam. As I sat at my desk stewing about the impending event, Skinner appeared at the door (first time ever, I believe), and immediately engaged me in conversation over some details of my first experiment. He then asked me how I had come up with the idea in the first place, and that led naturally to further possible work that might follow. I was just getting into this topic when he looked at his watch and said, “I think you’re warmed up now. Let’s go!”

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### Ogden R. Lindsley (1951–1964)

#### OUR HARVARD PIGEON, RAT, DOG, AND HUMAN LAB

Much, much, more than pigeon research happened in the west end of Harvard’s Memorial Hall basement in the early 1950s. We called both the rooms used by Fred Skinner and his doctoral students and their regular staff meetings the Pigeon Lab.

The Pigeon Lab suite had seven rooms. Skinner had his own and a secretary office. A small narrow room gave four graduate students desks. In a small shop we built and repaired prototype apparatus. In our supply room we found new apparatus parts and old apparatus to cannibalize. In our experiment room pigeons pecked away in their free-operant chambers. In their dormitory, pigeons waited in their home cages, superbly cared for by Mrs. Papp.

The informal pigeon staff meetings usually included the current Memorial Hall pigeon staff plus all the free-operant troops from central New England who could come to the seminar room in Memorial Hall on Friday afternoons (Skinner, 1983, pp. 26, 135). A sampling of those not working in Memorial Hall follows. Jim Anliker, Peter Dews, and John Falk, from Harvard Medical School. Mike Harrison from Boston University. Don and Pat Blough from Brown. Marc Waller from

Jackson labs in Bar Harbor, Maine. Barbara Ray, Paul Touchette, and Bea Barrett, from Fernald School, and my postdoctoral students and I from Metropolitan State Hospital.

While I was a German prisoner of war I promised myself that if I survived, I would spend half my life having fun and the other half studying people and trying to stop war. Back at Brown University I double majored in psychology and biology as a first step in stopping war by helping to build a science of human behavior. I yearned to move my research from rats to people. At Harvard, I saw Skinner’s pigeon experiments as even further removed from people than rats. But his methods were superb! So I introduced a rat demonstration to Skinner’s course, Natural Sciences 114, which Skinner and Harvard called “Human Behavior” and the students called “Pigeons.”

Figure 1 shows Samson Rat pulling down his weights. I designed and built Samson’s weight machine in our Pigeon Lab shop. Samson pulled several times his own weight demonstrating results of shaping to the students. They chose Samson as the class demo hit of the year! Samson’s acclaim caused his demonstration to endure in Natural Sciences





Fig. 1. Ogden Lindsley adjusts the weight while shaping Samson Rat to pull down up to three times his body weight for a class demonstration in Skinner's Natural Sciences 114 course at Harvard in 1952.

114 for a couple of decades. Ralph Gerbrands built a durable copy of Samson's weight-lifting machine out of stainless steel and brass in the Psychology Department shop.

Figure 2 shows Hunter discriminating while pressing a panel for dabs of ground hamburger. Of the 65 beagles that I trained for Boston University's total body radiation Atomic Energy Commission research project, Hunter learned most rapidly. I designed and built Hunter's prototype dog apparatus in our Harvard Pigeon Lab shop.

This research produced 10 products:

One, dogs got me closer to people than had pigeons and rats. Dog blood is so close to that of humans that research pharmacologists prefer to study it over other animal blood.

Two, we had brought another new species to the free operant.

Three, by using benzedrine, nembutal, and alcohol as well-known substance effects before radiating the dogs, we had socially valid standard effects to use in gauging the size of any radiation effects we might find.

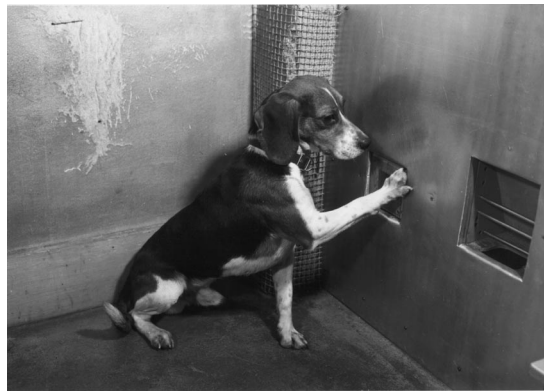


Fig. 2. Hunter, 2-year-old male beagle, presses a panel on a 1-min variable-interval schedule while watching for a dab of ground meat reinforcement to appear in the magazine opening to his right. Relocating the bars adjusted the opening to fit different dogs' noses. Hunter survived an LD50 dose of 300 roentgen units of total body irradiation in 1953 and became the Skinner family pet.

Four, we had built a stable 1-hr behavior sample including baseline variable-interval responding, a flashing light visual S<sup>A</sup> discrimination, and a buzzer followed by a horn blast conditioned auditory fear suppression.

Five, the LD50 (lethal dose 50% of the dogs died) of 300 roentgen units of radiation has a temporary immediate effect 1 hr after radiation extending the fear by disrupting the variable-interval baseline.

Six, only responding for food was disrupted as the dogs sickened and half died from maximum leukopenia (no white blood cells) about 15 days after radiation. The visual discrimination and sound-conditioned fear suppression continued without disruption until death (Jetter, Lindsley, & Wohlwill, 1953).

Seven, we first demonstrated using aversive loud noise for an aversive stimulus.

Eight, part of this research became my doctoral dissertation (Lindsley, 1957).

Nine, Nathan Azrin, a graduate student at Boston University, daily observed me working with the dogs and became interested, and we did a human social cooperation experiment together (Azrin & Lindsley, 1956). I designed the experiment by translating Skinner's three-key cooperating pigeon demonstration to human use by giving each child a wired stylus to act as a beak. I built the apparatus in the Pigeon Lab shop. Nate found a school, ran the students, and collected the data. Lat-

er I introduced Nate to Fred Skinner. Accepted as a Harvard graduate student, Nate did extremely well and got his PhD before I did!

Ten, Hunter, the brightest dog of the lot, survived to join Fred, Eve, Debbie, and Julie Skinner in their home on Old Dee Road, Cambridge (Skinner, 1983, p. 91)! Julie taught Hunter to pull a dog cart at their summer home on Monhegan Island.

Figure 3 shows our third daughter, Catherine Lee Lindsley, free panel pressing in her air crib. I built an experimental panel to put in one end of Cathy's air crib in our Pigeon Lab shop. I designed and built various hanging toys and flashing lights, which Cathy operated by pressing a signal on the lit panel at one end of her air crib. Sound proofed and child proofed, locked, and insulated, boxes under her crib held the cumulative recorder, counters, and relays that recorded Cathy's panel pushing to operate her toys. Our biggest discovery was Cathy's intermittent responding that gradually became more regular and even as she matured. These first human free-operant cumulative recordings were shared at a meeting of the Eastern Psychological Association (Lindsley & Lindsley, 1952<sup>1</sup>). *Newsweek* published a short note and photo describing Cathy's recorded play, which they titled "Babe in a Box." Several experimental psychologists attacked me for doing research on our daughter, and for depriving her of toys during parts of her day to see if it would increase her response rates when she had toy access.

Mine was not the only nonpigeon research spawned in the Pigeon Lab. Other Pigeon Lab folk pioneered with other species. But that is their story to tell. Those early 1950s were glory years at Harvard! We all wanted to prove the generality and sensitivity of our free operant by bringing in new species. I can still feel the excitement when we heard the rumor that Peter Dews had free-operantly conditioned an octopus while on a visit to Italy! In Hot Springs, Arkansas, Marion and Keller Breland were making a living training sheep, dogs, pigs, and chickens to perform astounding



Fig. 3. Cathy Lindsley presses a panel to flash lights and sounds on a fixed-ratio reinforcement schedule in her air crib in 1951. The bank of 10 colored reinforcing lights alternated above the panel, which had a discriminative signal light in its center.

ing tricks in county fairs! Joe Brady had finally found a reinforcer for cats (expired human blood!) at Walter Reed Hospital. For political reasons, the American Red Cross prevented Joe from publishing the results of using their expired blood supply!

You have just read how my rat, dog, and human free-operant research was born in our Harvard Pigeon Lab. Yes, many other wonderful and varied experiments grew from those few pigeons pecking keys in their boxes at 180 responses a minute!

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Frances K. McSweeney (1969–1974)

### THE MATCHING LAW ILLUSTRATES THE INFLUENCE OF THE HARVARD PIGEON LAB

Skinner, Herrnstein, and Baum were the faculty members associated with the Harvard Pigeon Lab during the time that I worked there. Skinner was accessible to students, but was not involved in the daily running of the lab. Herrnstein and Baum ran the lab and conducted the weekly discussions of research (the pigeon staff meetings). It was an exciting time because research on the matching law was in full swing. The lab was noisy with the clicking of the electromechanical relay equipment and the occasional scream of a graduate student who completed a circuit between a power bar carrying 110-V AC current and either a ground or a power bar carrying 28-V DC current.

Those working in the lab were also influenced by other members of the Harvard faculty. We were introduced to their work during the proseminar, a course that was required of all incoming graduate students. Herrnstein summarized the purpose of this course during our 1st day as graduate students. The purpose, he said, was to forge us into a unit through adversity, to allow the faculty to evaluate us relative to each other, and possibly to teach us some psychology. We spent an anxious evening discussing whether other schools would still be interested in our application.

The intellectual influence of the Harvard Pigeon Lab can be clarified by examining themes that appear in the work of the many people who were trained there. I'll use the matching law (Baum, 1974; Herrnstein, 1970) to illustrate these themes because it was the major topic of research when I was there. In many ways, the matching law represented both a continuation of work that had gone before and a profound departure from earlier work. The law also illustrates the influence of other Harvard faculty members. For example, the power law form of the generalized matching law strongly resembles S. S. Stevens' power law description of the psychophysical function.

#### *Empirical Laws*

With some exceptions (e.g., Killeen, Stadon), researchers trained at Harvard have formulated empirical generalizations about behavior rather than comprehensive theories. The matching law provides an example because it summarizes a large body of research but contains little theoretical explanation for these behavioral regularities. Additional empirical generalizations can be found in the work of many others trained in the lab (e.g., Fantino, 1969; Logue, 1988; Mazur, 1984; Neuringer, 1992; Williams, 1983). This empirical emphasis was a continuation of earlier work by Skinner and others. In my opinion, the empirical approach provides the most reasonable method for expanding our present knowledge. Given the limitations of our knowledge, the data do not adequately constrain elaborate theoretical speculation about behavior. The empirical approach, however, also places our field at odds with heavily theoretical areas of psychology (e.g., cognitive) and may have isolated and handicapped us in the competition with other fields for grant funding.

#### *Large, Orderly, and General Effects*

Researchers trained in the Harvard Pigeon Lab often study behavioral effects that are large, orderly, and general (e.g., Fantino, 1969; Logue, 1988; Mazur, 1984; Rachlin, 1973; Williams, 1983). Smaller, less orderly effects have a disconcerting way of vanishing when they are most needed (e.g., when you're trying to get tenure). Large and orderly effects also lend themselves to the precision of mathematical description. The matching law is an obvious example. It provides a relatively accurate mathematical description of a large effect. It is also highly general, describing the behavior of many different species, responding in many different ways for many different reinforcers. The only time that my own data failed to conform to this law, Herrnstein pointed out that I was using a changeover delay (COD) that was too

short. Sure enough, lengthening the COD cured the problem.

This particular approach to studying behavior has been criticized. For example, a famous psychologist, trained in a more theoretically oriented discipline, once told me that large and orderly behavioral effects are not necessarily theoretically important. Although I agree that occasionally a relatively small and fragile effect may have theoretical importance (e.g., blocking in classical conditioning; e.g., Kamin, 1969), I do not agree that a large and orderly effect can ever be theoretically unimportant. If one's theoretical goal is to describe and predict behavior, then one should concentrate on describing and explaining the large effects.

#### *Relative Measures*

While at Harvard, many of us learned that relative measures of behavior are usually more orderly and sensitive than absolute measures (e.g., Neuringer, 1967). As an example, the matching law has relative dependent and independent variables. Herrnstein argued that relative measures are more orderly because they control for many of the variables that create noise in the data. For example, if an animal's level of deprivation varies somewhat from session to session, then those changes will confound the effect of an independent variable (e.g., rate of reinforcement) when absolute response-rate measures are taken in different sessions. In contrast, fluctuations in deprivation will cancel when the effect of an independent variable is assessed by relative measures (e.g., the relative rates of responding for different rates of reinforcement within a single session).

#### *Molar Measures*

Although there are exceptions (e.g., Shimp, 1969), students trained at Harvard usually favor molar over molecular dependent and independent variables. Molar measures are taken over relatively long time periods; molecular measures are taken over smaller intervals. The matching law uses molar measures because its terms are measured over the entire experimental session. The emphasis on molar measures was heavily criticized when it was introduced (and today, e.g., Dinsmoor, 2001) because it represented a departure from Skinner's earlier use of cumu-

lative records. Our field will probably turn more and more to molecular measures as modern computer technology makes it easier to collect such data. However, even if all of the causes of behavior eventually prove to be molecular, the discovery of molar regularities in behavior will remain an important contribution of the Harvard Pigeon Lab. At the very least, molar regularities provide data for theories to explain.

#### *Conclusion*

I learned a great deal from my time in the Harvard Pigeon Lab. I've cited the work of Herrnstein and Baum because the matching law illustrates many relevant themes. I should also note that Skinner was generous with his time and was always available for a chat with students. One lesson that I learned particularly well was how to hurdle. Herrnstein often commented that the faculty didn't know how to teach students anything, but they did know how to place hurdles between students and their degrees. One of my fellow students observed that, by this thinking, students should be required to climb William James Hall (12 floors or so) rather than to take preliminary examinations. Herrnstein agreed and commented that the results would also be easier to grade. By Herrnstein's thinking, students who succeeded at Harvard would learn how to overcome obstacles. Most of us learned that lesson quite well.

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## Harold L. Miller, Jr. (1971–1975)

### QUALITATIVELY DIFFERENT REINFORCERS IN THE HARVARD PIGEON LAB

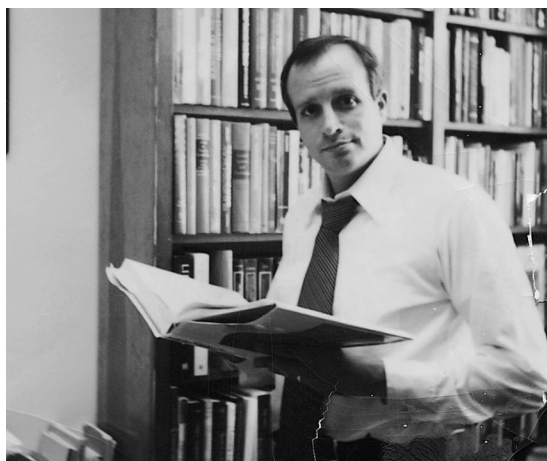
I owe my place in the Pigeon Lab at Harvard directly to Peter Killeen and to the anonymous graders of the qualifying exams (the notorious prelims) administered at the end of my 1st year as a Harvard PhD student. To Peter because he invited me to join his newly established lab at Arizona State University when I was a junior there and allowed me to collaborate with him in research on a qualitatively different reinforcer: light. He was mentor as much as collaborator and encouraged me to put Harvard in my sights. No doubt his role in recommending me made a large difference to my admission. And to the anonymous graders because passing the exams made it possible for me to stake out a place in the lab. At the time, there was a strict policy of commencing one's research only after the exams had been taken (and passed).

My 1st year at Harvard brought me into contact with Dick Herrnstein, whose graduate seminar, Motivation and Action, was to prove pivotal to my subsequent research. My adviser in that year was Billy Baum, distinguished by lengthy beard and wall-covering poster of Maher Baba, and, like Dick, degrees only from Harvard. Although Peter had first acquainted me with the matching law, taking Dick's seminar and assisting Billy in his undergraduate learning course drove the acquaintance deeper and to the point of inspiring research projects I could call my own. I recall Dick mentioning all sorts of ways in which the matching law could be extended (on both sides of the equation) and practically begging that matching be studied in an experimental arrangement involving choice between different kinds (qualities) of reinforcers.

The seventh-floor (William James Hall) lab that I entered in my 2nd year was storied, not least because of the list of those who had completed dissertations there (and in the precursor labs elsewhere on campus) while using virtually the same equipment that was still in place, and the fact that Fred Skinner's office was adjacent. He had retired before I arrived but was still a frequent presence (in his office but never in the lab) and, as the object of visits from notable guests and media from around the world, very much a celebrity. The lab proper occupied as many as 10 rooms of various size, including colony rooms for individually housed pigeons and rats (and one presiding crow), rooms containing experimental chambers, and rooms housing the apparatus for experimental control—rows of relay racks that reached floor to ceiling. Later a new gadget—a PDP-8<sup>®</sup> minicomputer—made its appearance in the lab and, in tandem with the programming language known as SKED<sup>®</sup>, revolutionized the way we conducted research. The rooms containing chambers were linked to those containing the control equipment by bundles of cables that wound their way through walls, above the ceiling, and along the floor. The whole scene gave the distinct impression of wire world gone amok. When animals were active in all the chambers, there was an attendant cacophony of click-clacking, whirring, buzzing, and so forth that added to the head-spinning sense of order on the verge of welter.

My first task was to self-learn the relay circuitry (Peter's lab had been Digibit based); a rite of passage, it seemed. Electrical shorts and more than a few shocks were part of the





Harold L. Miller, Jr. (circa 1972).

experience in an environment that mixed AC and DC circuits. I recall the elation that came with finally programming a VI schedule, which proved a secondary feat compared to the nightmare that was a concurrent VI VI schedule with a changeover delay. There were endless simulations of the procedure at the relay rack, followed by shuttling back and forth from rack to chamber in an effort to ascertain that everything was happening just as it should before bird (or rodent) was ever drafted for service. Even then, I was no stranger to the sinking feelings that came with subsequent discovery of oversights in the programming or unnoticed failures of the equipment.

My primary associate through the thick and thin of 4 years' research was Will Vaughan. We shared an office, and conversations there formed the primary substance of my graduate education. With Will's help I designed experiments, wired them, ran them, and made sense of the results. We traded off running each other's subjects: I in the early morning and on Saturdays, he in the evenings and on Sundays. In between they were run by two pillars of the lab: Kitty Papp and Ginny Upham.

My research required the modification of pigeon and rat chambers to include a pair of grain hoppers or liquid dispensers instead of the one that was standard equipment. I arranged the purchase of several types of grain for use with pigeons and produced several concentrations of sugar water and sweetened condensed milk solutions for use with rats. These became the qualitatively and quantitatively different reinforcers in a variety of concurrent VI VI arrangements, probably more than 20 separate experiments by the time my graduate career concluded. Two of them figured in my dissertation (which Dick advised); one of them was subsequently published (Miller, 1976). The upshot of these variations on a theme was a method for the measurement of reinforcer value—hedonic scaling—premised on deviations from matching to reinforcement rate alone.

I typed my dissertation using a nonelectric Smith-Corona portable; all the figures were hand drawn. After the dissertation defense in June 1975, my family and I moved to Utah. On the day before we left, I dropped by Dick's office for a final chat. He complimented the dissertation and wished me well. I asked him an odd question: Did he have any recollection of why I had ever been admitted to the program? He mentioned Peter's endorsement, then added that an item in my record—namely, attending a junior college in Florida—had reminded him of summers he had spent in military consulting at an Air Force base near the college. He figured it as a good sign. From such subtleties of contingency are graduate careers made.

#### REFERENCE

- Miller, H. L., Jr. (1976). Matching-based hedonic scaling in the pigeon. *Journal of the Experimental Analysis of Behavior*, 26, 335–347.

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## John Staddon (1961–1964)

## MEMORIES OF MEMORIAL HALL

I entered the basement of Memorial Hall, then sans its elegant spire, destroyed much earlier in a fire (but now, in the fullness of Harvard's Croesus-proportioned endowment, fully restored), in September 1961, as a 1st-year graduate student in experimental psychology. It was not the first time. I had visited earlier in the year as a prospective student and was shown around most hospitably by a young faculty member, R. J. Herrnstein. What I remember of that first visit was seeing visible evidence of the fact that it was possible to study the behavior of individual animals in a direct and powerful way—no statistics, no group data. The lab that most impressed me belonged to an Australian graduate student, Peter van Sommers. Peter was a genius with equipment and a beautiful experimenter. He was studying the effect of oxygen reinforcement on the behavior of goldfish, as I recall, and I was most impressed to see the little fish swimming upstream in a Plexiglas tube and nosing a plastic disk that delivered pulses of oxygenated water.

The physical environment, the basement of Memorial Hall, was inelegant—almost every room had a ceiling full of heating pipes—but wonderfully efficient. With more than 100 rooms, all on one level, for no more than half a dozen faculty members (including the illustrious but somewhat reclusive George von Békésy) all was accessible and there was no shortage of space. Students were housed in groups of six, and my office was right next to the small but well-organized departmental library. As a student, the efficiency of the place struck me with great force. There was an excellent wood and machine shop; drawings were produced by a lady who worked outside S. S. ("Smitty") Stevens' office; and the Pigeon Lab was a positive factory for generating data. Run by the odd couple, Wally Brown and the formidable Mrs. Antoinette C. Papp—footnoted in dozens of *JEAB* papers



John Staddon (circa 1962).

from that era—all that was required to get an experiment going was to wire up the apparatus, label the counters whose readings were to be noted each day, and show Mrs. Papp or Wally the "on" button. With this wonderful system, a student might have three or four experiments running simultaneously.

The most active faculty member at that time was the late and very much lamented George Reynolds, then an assistant professor. Every week (or so it seemed) everyone would get in his or her mailbox a green-covered<sup>1</sup> *JEAB* paper by George, either on his own or in collaboration with Charlie Catania, then a postdoc. Also circulating was a fat mimeographed manuscript entitled "A Quantitative Analysis of the Behavior Maintained by Interval Schedules of Reinforcement" (finally published in 1968) that was a veritable mother lode of information on the role of temporal control in reinforcement schedules. Surely, I remember thinking, we are very close to understanding how all this works.

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<sup>1</sup> Grant money was generous then; one could afford the covers.